

Social sciences as learning systems

Smelser, Neil J.

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Neil J. Smelser

Social Sciences as Learning Systems



WZB Lectures

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Neil J. Smelser

Social Sciences as Learning Systems

Keynote Address
at the WZB Conference
chaired by Ariane Berthoin Antal
and Meinolf Dierkes on
"Organizations in the 21st Century:
Knowledge and Learning—The Basis
for Growth"

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WZB Lectures

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Wissenschaftszentrum Berlin
für Sozialforschung (WZB)
D-10785 Berlin-Tiergarten
Reichpietschufer 50
Telephone: (+49) 30 25 49 1-0
Telefax: (+49) 30 25 49 16 84
Internet: <http://www.wz-berlin.de>

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Social Sciences as Learning Systems

Prologue

I should begin by telling you that I did not invent the title of my remarks today. Furthermore, you should know that when I first heard it, my initial response was to reject it and question the sanity of the organizers who suggested it. I simply didn't know how to gain a foothold in addressing it. However, the more I thought about it the more I came to realize how the analogies of individual and organizational learning and adaptation can lead us to understand and appreciate many of the things that happen in our fields. So I decided to give it a try, and I am pleased to put the results of my efforts before you today.

My initial reaction should, however, provide a cautionary note to be kept in mind. We should take care to guard against a reification of the question. A social science—to say nothing of the social sciences as a whole—is not a real thing, a creature that learns. It is made up of multiple disciplines, areas, groups, and ongoing processes, which are, in turn, made up of thousands of individual persons. Furthermore, generalizations are difficult to come by. Nevertheless, I am confident that there are shared styles of learning among scientists, and that definite patterns of learning emerge. It is those patterns, the product of constantly recurring processes, on which I will focus.

Scientific Learning, or Learning to Focus

Most of the social sciences—always with variations, as will be the case throughout these remarks—came into existence and experienced their early growth under the vision and legitimizing cloak of positive science. This meant above all that their search was directed toward discovering *general* knowledge and laws: laws of evolution, laws of economics, psychological laws, social laws. As often as not, some natural-science model, such as Newtonian physics or functionalist biology, was at hand as an image to emulate. Some dominant organizing focus—in effect, but not then called a master paradigm—for the emerging disciplines was sought, and some notion of the scientific *unity* of knowledge was also among the aspirations.

Given such orientations, one would expect—retrospectively—that the guiding principles for investigation would be the search for and demonstration of social-science laws. There has been evidence for this kind of pursuit, even to this day. But it has not been the main story; in fact it has been a receding story. Instead, social scientists have worked in the direction of *particularizing* their knowledge into smaller and more specialized subfields, theoretical perspectives, and research traditions, while still maintaining sometimes that general principles—such as supply-demand, psychological reinforcement, or social conformity—are at work. But in reality there is an economics of this, a sociology of that, and an anthropology of this and that. Proliferation and specialization have always increased, never decreased. The overall effects—contrary to the early ambitions—have been to *disperse* knowledge, to *qualify*, *modify*, and *make contingent* general principles, and to *fragment* rather than *unify* knowledge (Levine, 1994; Smelser, 1974).

The learning involved here is learning to pursue more particular topics and problems—opportunities if you will—and social scientists have proven themselves to be very good at this. What have been the mechanisms involved in this kind of pursuit?

- Some pressure to specify and particularize knowledge has arisen in the normal course of scientific inquiry. Empirical investigation always turns up the result that general laws do not work; the empirical subject-matter of society inevitably turns out to be too complex

and stubborn to permit easy demonstration of laws. Some social scientists will also remain stubborn and insist on the validity of their general principles—i.e., refuse to learn—much more on this tendency later—but in most cases they will learn by qualifying, seeking alternative explanations, and searching for new perspectives. The end result of this process, of course, was particularization and diversification of knowledge.

- The subject-matter studied by social scientists is itself becoming more complex all the time. It is no secret that the rise and growth of the social sciences coincided historically with the great commercial and industrial revolutions beginning in the eighteenth century. These revolutions meant above all a corresponding differentiation and complexity of society and social arrangements. In some respects this proved a blessing for the emerging social sciences. It has been suggested, with good reason, that formal economics owes its existence to the emergence of a differentiated market economy, and that political science and sociology, respectively, could not have crystallized as separate disciplines without a corresponding differentiation between state and society. Yet this same process of differentiation rendered their subject-matter increasingly more complex, by producing multitudes of identifiable phenomena—institutions, organizations, groups, and processes—all emerging as identifiable *and* worthy of study. I argue, therefore, that the differentiation and diversity of the social sciences is, in part, a mirror of the same processes in society, which forever produces new things to understand and explain.
- Among the phenomena produced by rapid and irregular change, moreover, are an increasing array of social problems—unemployment, poverty, crime, family instability, and others. Many of these, such as prostitution, were old; many, such as the emergence of labor-management strife, were new; and many were old but have become problems because they come to reside in new social contexts or because new and more exacting social and moral standards *make* them social problems when they were not before—for example, slavery and child labor. In all events, because the social sciences were born in the context of faith in material and social progress, all of these sciences—in their separate ways—are inclined to be sensitive

to anomalies, imperfections, flaws, injustices, and problems in society. By virtue of the same sensitivity, moreover, scholars have been drawn to study them in hopes of ameliorating them. These social problems themselves become foci of subfields—the economics of poverty, the sociology of poverty, the politics of welfare, and so on—thus producing new specialties and specialists.

- Another form of learning (adaptation might be a better term) is the exportation and importation of concepts, theories, frameworks between disciplines—also known as imperialism and lack of scientific confidence, respectively, by those who sneer at the processes. All disciplines export and are borrowed from, though the flow varies. I would suggest that economics is a net exporter (mainly of market models, rational choice models, and formal quantitative models), that geography is a net importer (Marxist geography, political geography, feminist geography, postmodern geography, queer geography), and that sociology, anthropology, and political science are intermediate—though political science has had heavy seasons of wholesale importing of behaviorist approaches, functionalist approaches, and rational-choice and game-theoretical approaches.

Whatever the precise picture, exporting and importing are widespread phenomena. Willard van Orman Quine, the late philosopher who did not love the social sciences very much, once compared them to the Cargo Cults of Melanesia—that is, groups of despairing peoples who are forever waiting for magical arrivals of ships filled with unlimited supplies in the form of tinned foods, transistor radios, and personal salvation. The unflattering analogy was that social scientists seek their salvation by waiting for new theories, models, and insights from outside. While caricatured, Quine's dig struck a nerve and was not altogether out of touch with reality. Exporting and importing are, of course, one form of interdisciplinarity, and two of their consequences are to make the social-science disciplines more diverse internally and less distinguishable from one another.

- Closely related to exporting and importing are what Mattei Dogan (1990) and others have called hybridization, or the proliferation of new subspecialties by the selective fusing of ingredients from several

disciplinary sources (Dogan and Pahre, 1990). Child-development is a ready example, having ingredients of psychology, sociology, and biological perspectives. Behavioral economics—fusing economics and psychology—is another. Hybridization is, of course, another way of making for diversity and complexity.

- Two more sources of focussing are perhaps less noble, but nevertheless have to be mentioned. The first stems from the fact that social scientists are implicated in prestige systems—as are all scientists and scholars—and this means that they want to be noticed by colleagues and others. Being noticed, of course, derives in part from excellent scientific work, but it can also come from novel formulations that capture the moment, from scathing attacks on leaders of the field, and from igniting academic and political controversies. These produce the “fads and foibles” that so exercised the sociologist Pitirim Sorokin (1956)—who, indeed, added to his own recognition value by attacking them so savagely. They also produce “schools” and “sub-schools” of thought, some of which have lasting value. These kinds of activity, too, add to the disagreement and conflict, and subtract from the consensus of academic disciplines.
- The second is the constant changing patterns of opportunities to conduct research supported by external grants. I noted earlier that academic disciplines tend to be sensitive to changes in the economic, social, and political environments in which they live. But they are not the only ones. As governmental agencies have proliferated and as private foundations have continued to grow, these agencies make it their business to be sensitive to concrete social problems and opportunities. One of the ways they implement their sensitivity, moreover, is to offer funds, sometimes in very substantial amounts, to social scientists who, they believe, will contribute directly or indirectly to social improvement by conducting relevant scientific work. When Requests for Proposals go out, moreover, we witness instances of demand creating its own supply of “expert and qualified” persons ready to offer promises and receive funds that are offered. At the deviant extremes, we see scientists who shape their proposals in the direction of the purposes for which the support is intended, but skew the work in their own

independent directions. The collective impact is to create new hives of activity, new groups of scientists, and sometimes new subfields—by external acclaim, as it were.

This long list of mechanisms that produced splintering and diversity is meant to underscore my first fundamental point: that social scientists learn quickly and respond quickly to the opportunities in their diverse environments, and the result of this activity is an enormous and continuous *spawning* of their subject-matters. I pointed out earlier that the net effect of this is to take them further and further from their earlier mission to produce *reliable, systematic, general knowledge*. I can say this straightforwardly, without contradicting the equally valid truth that the quality of specialized knowledge continues to improve because of both conceptual, methodological, and technical innovations.

However, social scientists have not learned—and it becomes increasingly difficult to learn—how to put back together what they have proliferated. All social-science disciplines still *do* theory in their fields, and they *teach* theory to their students and trainees in courses and in conducting research. But theory itself has become more subspecialized, and in some disciplines theory courses and textbooks teach *types* of theory, or if they teach what they call general theory it often seems to be partial in relation to their entire disciplines, and increasingly unrelated to the specialized, necessarily somewhat eclectic activity undertaken in concrete empirical research. This situation is a general one, though I of course acknowledge that it applies more forcefully in some disciplines than in others.

The main type of integrative work that currently goes on in the behavioral and social sciences is the *interdisciplinary* focus on a given area of study, intellectual problem, or social problem (Centre for Educational Research and Innovation, 1972). Sometimes this kind of activity crystallizes into a distinctive subspecialization—such as organization studies—which is really not identifiable with a single discipline, and in the case mentioned, is often located elsewhere, especially in schools of business and management. Sometimes interdisciplinary approaches are housed in organized research units peopled by faculty from different departments. Sometimes these interdisciplinary efforts yield mini-professional societies, as in the case of the group on network analysis, or in the many associ-

ations focussing on area studies. Great value comes from these interdisciplinary societies—or interdisciplinary learning, if you will—but it is a rather specialized form of integration.

At the same time the hankering for some kind of general science and unity of knowledge persists. Some scholars try to advance general theories, and do. This hankering no doubt goes back to our historical roots. Certainly it lies behind the periodic declamations, in discipline after discipline, that all is hopelessly splintered, beyond organized comprehension, or, at worst, in crisis.

I cannot conclude this section without asking a few questions of you. Is not the impulse to integrate—to learn what it is all about—now obsolete, given the history of explosion, differentiation, and fragmentation of knowledge I have described? Is it not a form of romanticism, a harking back to earlier visions of the unity of knowledge, which did not happen then and certainly are much further from happening now? Is not the hand-wringing about the fragmentation and crisis some elaborate form of self-torture, expecting or hoping for something that was never meant to happen? Should we not, finally, change our expectations instead of being continuously disappointed that disciplinary unity seems further and further from being realized? Maybe the directions implied by these questions are not the way to go, but if we were to go that way we might be a little happier with life.

Organizational Learning and Non-Learning

Thus far I have focussed more or less exclusively on the knowledge side of the social sciences—the directions of learning, if you will—and some of the consequences of the patterns I have identified. This scientific work, of course, must take place in some kind of structure of institutions, organizations, and social roles—though this social structure displays many variations. I refer to the departments, faculties and academies, the professional associations, the sources and forms of financial support and the professional academic roles in which most social scientists are incumbents. This is what we call infrastructure. While many features of this infrastructure have been devised—or have evolved—as ways of encouraging scientific work and creativity, their actual effects are of a mixed character. This portion of my remarks will be some reflections on these effects.

The first observation to be made about this infrastructure is how relatively stable it is over time, in contrast to the dynamism and flux of the knowledge produced by the scientists that inhabit that infrastructure. The universities, divided into faculties according to branches of knowledge, are the most ancient of institutions, and despite the great subdivisions of faculties into academic departments, the university *form* has persisted. Departments or faculties are associations of professional peers, the limits being defined by academic subject-matter. They are finely graded by rank and prestige, but in general are granted a great deal of freedom and are subjected to limited authority—the latter varying considerably along lines of national and political tradition.

The professional associations are created and peopled by groups, and evolve into similar structures with rather similar functions from field to field—to define and endow professional identity, to serve as status-protecting and status-advancing bodies that sometimes lobby politically, to exchange knowledge, and to gather periodically in collective expression of common membership. Academic roles are also typically privileged roles accorded with a high level of political freedom and job security (“tenure”), and those who are successful in these roles are rewarded mainly with much prestige, but seldom with much wealth or power. It would no doubt be wrong to describe these forms of institutional and organizational stability in terms of inability to learn organizationally,

because the structures are so resistant to change even if individuals and groups might want to change them. What are the forces that make for such structural stability? I do not think we know, but I will mention two factors for your consideration.

- The first factor is cultural. Much of the university system has its origins in the church, and despite a long-term process of secularization that obscures that connection, universities and colleges retain a flavor of the sacred, in part because of their role as creator, guardian, and sometimes critic of national and civilizational culture, and in part because they share responsibility for the education of the society's younger generations. In so far as they partake of the sacred, they have an interest in preservation, in tradition, in ritual, and in continuity. I do not want to overstress this factor, but want to mention it because we academics, being mostly secular, if not anti-religious in outlook, tend to overlook it.
- The second factor is political-economic. Being embedded in universities and academies with a multiplicity of departments and faculties, these units are budgeted as such, and are in the nature of the cases pitted against one another in competition for resources. They represent themselves as scientific or scholarly disciplines, important in the life of the college or university, and thus meriting resources for maintaining themselves, their numbers, their students, and their space. It is incumbent upon them to represent themselves as such, because different, more transient units are notoriously more vulnerable from a budgetary point of view. In a word, faculties and departments—and in the larger society, professional associations—are deeply implicated as vested interests in a system of competition among multiple academic interest groups.

Now I turn to a few observations about the implications of these structures for scientific learning and creativity.

First, the department and the discipline. Here we have a mixed story. Quite obviously these are the seat of the education and training of future professionals, and, as such, the creators of the human capital on which future scientific work and creativity depends. At the same time faculty members are notoriously interested in cloning students into people

like themselves, so that one aspect of professional training is to pass on not only knowledge, but also their own preoccupations, issues and “hang-ups” to those they train—a distinct source of intergenerational non-learning. (Much more on “hang-ups” later.)

More generally, the power of disciplines themselves constitutes a constraint on looking outward beyond disciplinary boundaries. The careers of the young are in the hands of their disciplinary seniors who hire them, evaluate them, and vote on them for advancement. Under these circumstances it is perilous to challenge too much or wander too much. One of the universal human motivations is the yearning for immortality, and in the academic world this yearning is felt to be realized by having one’s students carry on one’s work. The collective impact of this is inevitably a conservatizing influence.

Second, we should look at the same process from the standpoint of academic tenure and the typical career contour of the professional social scientist. The picture is once again mixed. On the one hand, the institution of tenure is a powerful guarantee against the intrusion of exterior political forces in the lives of professionals—this is the core feature of academic freedom. Furthermore, when tenure carries with it a comfortable income, it provides the social scientist with year-to-year protection from distractions from his or her work occasioned by the need to find other paid work. Such are the benefits. This system of supporting professionals in universities and academies—supplemented by scientific workers in industry and government—has created unprecedented armies of full-time supported seekers and creators of knowledge.

Careers in scientific work, however, may be inhibited or deflected by the very apparatus that is meant to encourage them. The socially-accepted motivations for scientists is that they be committed to a calling—again, of a quasi-religious nature—and that they may strive for recognition and prestige. At the same time the motives for security and continuity are among the repertoire of considerations affecting academic careers.

As mentioned, the pre-tenure years have their pressures to conform to expectations of one’s seniors, however much the ideology of originality and creativity is officially voiced. Too often—at least in the American setting I know—decisions are made on the relatively “safe” criterion of

advancing young scholars on the basis of numbers of publications in prestigious refereed journals, which becomes a rather stylized process, encouraging young scholars to assume a calculative, incremental orientation toward organizing his or her research.

After tenure is granted, the scientist, even if inhibited in the struggle to attain it, is in principle liberated from that struggle and free to move in new directions. In many instances this is precisely what happens in the mid-career and senior years. At the same time there are two counterforces to this development. First, we might say that the damage has already been done, if I might overstate the case. For many, pursuing a later career means continuing along the lines that have been cemented in the younger years, as if the law of psychological reinforcement is working itself out. Second, the principle of evaluation by seniors may continue, as scholars continue to work their way toward the top.

While on this line of reasoning, I cannot resist mentioning the subject of peer review, which has long been institutionalized as a mechanism for career advancement in college, university, and academy settings. It has become more widespread in recent times, as government agencies and foundations have embraced the support of research in the social sciences as part of their missions, whether out of the increasingly-perceived need for knowledge as a basis for enlightened policy or out of the lingering conviction that understanding—scientific understanding—is a necessary ingredient in social amelioration and social improvement.

The logic justifying peer review is that the most effective way to assure quality in research supported is to rely on the advice of experts—those who have been active and successful in the relevant arena of research. To choose the best who have achieved is a way of securing the best talent among those who are aspiring. In practice, those agencies who are responsible for granting research support are themselves motivated to do their best, so they choose evaluating peers on a safe basis, relying mainly on the known or reputed status of the prospective reviewers. Defensible as the practice is, it does serve to reproduce the whole system of evaluation that I have described, and runs the risk of assuring the continuity of scientific correctness. For this reason and others, the system of peer review has come under attack as a self-serving mechanism for science. The irony, however, is that it tends to endure because superior alternative

methods are difficult to devise without falling back on ignorance or opening the door to non-scientific criteria—including political criteria.

I have reviewed for you several social-structural sources of non-learning in the social sciences, some of which apply more widely in academia. Actually, what I have described is a combination of structural arrangements and the playing out of human failings. I will readily admit two of *my* failings in putting forth this diagnosis. First, if you ask me to provide measures of the diverse tendencies and their impact on learning in the social sciences, I will tell you that such measures are not available, and that my case rests on a mix of logical analysis and a lifetime of experience in academia and the “research establishment.” And, second, if you ask me to produce a range of reforms that will make for a less imperfect system, I will disappoint you on that count, too, beyond saying that collectively we as social scientists have to be ever-diligent to keep our minds’ procedures open so that *both* the scientifically correct *and* the scientifically high-risk but potentially high-payoff activities find nurturance in the system.

Sectarian Tendencies and Non-Learning

Twice in my remarks I have referred to religious or quasi-religious ingredients in the social-science enterprise. In this third and final section of my remarks I will pursue an even more explicit religious analogy, bring out the issues of faith, orthodoxy, sectarian conflict, and schismatic tendencies—all of which come to constitute serious obstacles to scientific learning and advancement.

One of the early scientific aspirations of the behavioral and social sciences is that they would accumulate knowledge in the same way that the natural sciences were thought to accumulate. The essence of that model was that through constant experimentation and discovery, science continuously builds on its own past but, at the same time, *discards* that past as prior knowledge is shown to be erroneous or limited. As a result, the study of the history of science is largely a matter of curiosity. That, we might say, is the classical model of how learning occurs in the natural sciences.

Whether knowledge in the natural sciences accumulates according to that script has become a matter of continuous controversy, especially in the era that began with the publication of Thomas Kuhn's *The Structure of Scientific Revolutions* (1972) and extending right up through the continuing post-modernist assault on science from its vantage point of a radical epistemological relativism.

Be all that as it may, the developmental picture in the behavioral and social sciences is not that of the earlier model of knowledge accumulation. Rather, it is a story of continuous invention of new or revived theories or perspectives which capture the imagination of a subclass of scientists, and occasion a season of research activity and the consolidation of approaches as "schools" or "approaches." At a certain point in this process, however, criticisms of the approach appears, and leads to an appreciation of its errors, limitations, and biases. As often as not these critics are inclined to invent new frameworks designed to supersede the outmoded old one. The new approaches, moreover, become "schools" or "approaches" of their own, and they too become vulnerable to the same dynamic of invention, elaboration, consolidation, and attack.

The peculiar character of this dynamic is that the older approaches seldom die altogether. Some do, but more often they persist or go underground for a time, only to reappear in altered form, which then perhaps gets the word "neo" in front of the older name and enjoys a new season as a school. This whole process, repeated hundreds of times in the history of our disciplines, yields an accumulation of sorts—in richness and diversity of paradigms, schools, approaches, and sub-approaches. But it is not accumulation of successive discoveries and discardings of the past on the basis of these discoveries. Furthermore, this process, like those I sketched out at the beginning of my remarks, contributes to the diversity and dispersion of the behavioral and social sciences as more or less coherent paradigms.

To make this process more concrete and visible, let me quickly enumerate a few notable paradigms:

- The eclipse of classical evolutionary theory in anthropology and sociology by the diffusionist approach (which challenged the linearity of change processes) and anthropological and sociological functionalism (which stressed contemporary systemic interrelations of structures in society, rather than evolutionary stages and survivals).
- The subsequent attack on functionalism on the grounds that it could not explain conflicts and contradictions, and that it was ideologically conservative. This occurred mainly in the 1960s and 1970s, and saw the simultaneous rise of conflict sociology, radical sociology and a renewed stress on micro-sociological approaches as symbolic interactionism and ethnomethodology.
- In economics, the Keynesian critique of classical equilibrium theory, the consolidation of Keynesianism, attacks on the limitation of Keynesian theory, the development of varieties of neo-Keynesianism, the separation of distinct new lines of monetary and fiscal policy economics, and the emergence of distinctive "supply-side" and "demand-side" economics.
- I have already mentioned the assault on traditional political theory

and comparative historical analysis of political institutions as “unscientific”, the simultaneous rise of the behaviorist and functionalist emphases to take their place in the 1950s and 1960s, and a similar “rational choice” revolution in the 1980s and 1990s.

As mentioned, the net result of these processes—visible in all the behavioral and social sciences—is a kaleidoscope of competing paradigms, theories, and approaches.

One aspect of these dynamics—and here is where the religious analogy comes in—is that social scientists do not usually accept the idea that these various approaches might comfortably co-exist, side by side, as so many alternative ways of pursuing knowledge. Far from it. Instead they tend to *identify* themselves with the paradigms they favor, and to *denigrate* the approaches they regard as *inferior*. It is only a small step to begin to endow their own approach with a certain *sacred* quality, and as soon as this step is taken, the stage is set for the periodic outbreak of holy wars among the competing sects and cults. Perhaps I exaggerate here, but not too much. Sectarianism in the social sciences involves the drift of different theoretical and methodological preferences toward sacred objects, which believers embrace with fervor. Simultaneously they lash out at other approaches—as critics of their own—with equal, negative fervor. The net result is groups of social scientists talking among themselves and not much to others.

Vivid illustration of all this can be seen in Norman Cantor’s book review of a book on medieval plagues that appeared recently in *The Chronicle of Higher Education*. In the course of his review he turned his sights on his fellow medievalists:

My colleagues among American medievalists always remind me of those Boer farmers in South Africa in the 1860s and 1870s, trying to gain a living from their hardscrabble farms on the veldt, while a dozen or so yards beneath the soil lay some of the largest diamond and gold deposits in the world. So it is with academic medievalists—exclusively addressing only each other in conferences, journal articles, and university-press books, while there is, I believe, great and unsatisfied interest in the Middle Ages among the educated public (Cantor, 2001).

To turn this line of thought to the main purpose of the occasion, such developments foster *non-learning* in the extreme by shutting out other influences as foreign and shutting down lines of inquiry and research outside their restricted groups. Looking at it from the standpoint of rational, open, and scientific pursuit of knowledge, it seems outright insidious.

To push this religious analogy one step further, I might point out that different social sciences have developed different modes of handling sectarianism, controversy and heterodoxy in their midst. Economics seems to have evolved a mode that resembles that of the Catholic Church. For example, when the classical mode of perfect competition was challenged in the 1930s by Joan Robinson and Edwin Chamberlain, the theoretical outcome was to alter the parameters of market behavior, *and then* to calculate how economic actors would behave within *that* framework. Similarly, when the classical assumption of complete market information was challenged by those appreciating the importance of risk and uncertainty in the market, the principal outcome was to build models of rational response to risk and uncertainty situations. When the principle of rationality was challenged by Herbert Simon, this triggered a tradition in economics, political science and organization studies known as *bounded* rationality. The connection with Catholicism is not with its long tradition of repressing heterodoxy cruelly and sometimes violently, but in the formation of orders within the Church. In that connection, the principle was one of confronting divergent and heterodox tendencies—Jesuit, Benedictine, etc.—by institutionalizing them as orders, which are then regarded as variations within the continuity of the basic faith in the Church.

Anthropology and sociology have taken an alternative path. Perhaps never as coherent as economics from the beginning, the dominant pattern of accommodation is to permit schismatic splitting-off of new approaches into schools, without apparent attempts to incorporate or reunite them with the larger disciplinary tradition, except in name. This is the pattern of schismatic secession of sects, which is so typical of Protestant Christianity. That tradition, born out of the *rejection* of the sacredness of holy authority, found itself crippled when it came to handling internal conflict by the enforcement of authority, and corre-

spondingly ill-equipped to handle conflict in ways other than having the warring parties hive off from one another into separate organizations.

Political science is still another variant. While resembling anthropology and sociology more than it does economics, many political scientists have become fixated on democracy as a political system. Political theorists attempt to identify the theoretical *essentials* of democracy, while an army of comparative political scientists attempts to identify how different national cases deviate from that theoretical model, in the process they identify many *subtypes* of democracy. This might be said to resemble the tradition of Platonism, or the pursuit of an idealized reality of pure types, none of which are realized in this earthly, imperfect world.

So much for these religious aspects of our quest for knowledge. As a final illustration of “non-learning” associated with these aspects, let me point out the tendency for certain philosophical and methodological tensions to repeat themselves in the social sciences, without any apparent learning emerging from the periodic confrontations. Because my time is so short, I will only mention a few such controversies in the hope that you are sufficiently familiar with them to recognize the phenomena I am underscoring:

- The struggle between the principle of “original sin” in theoretical formulations vs. the assumed possibility of human betterment. Freudian psychoanalysis and its theory of the instincts captures the essence of the former, and the progressivist impulse in the late nineteenth and twentieth centuries is an example of the latter. Marxism captured both in its theory—the inherent evils of capitalism and the promise of purposive human redemption and perfection in revolutionary communism.
- The possibility of a general science of society vs. the antagonism to this found in various expressions of historicism.
- Positive science vs. the relativism of knowledge.
- Quantitative vs. qualitative methods as roads to knowledge.

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Value-neutral vs. socially and potentially engaged knowledge, one version of which is positive vs. normative economics, another of which is scientific vs. reform sociology.

These constitute a final form of non-learning, a set of repetition-compulsions in our midst. We sometimes forget that they are old and recurring, but think that they are something entirely new and original and exciting. In any event we do not seem to learn from them. I personally have the hope that one of these years we will find ways to break through these repetitive conflicts and fashion out new theoretical and methodological positions that render them moot and not worth fighting over. I fear, however, that this is a vain hope for the foreseeable future, and that the more likely scenario is that they will continue to occupy our attention as one ingredient in this panoply of learning and non-learning principles in our ongoing scientific strivings.

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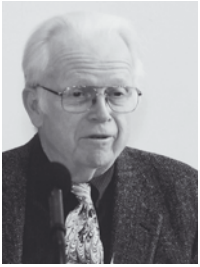
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The Author



Neil J. Smelser

Born 1930 in Kahoka, Missouri, Ph.D. in Sociology 1958 (Harvard University), psychoanalyst 1971; Professor of Sociology, University of California, Berkeley; Director, Center for Advanced Study in the Behavioral Sciences, Stanford (1994–2001). Numerous professional activities and memberships, e. g. Editor, *American Sociological Review*; Chairman of the Board, Social Science Research Council; Member, Council of the American Academy of Arts and Sciences; Member, Board of Trustees, Russell Sage Foundation; Presidential Advisor, Commission on the Causes and Prevention of Violence; Vice President, International Sociological Association. Co-editor-in-chief with Paul B. Baltes, “*International Encyclopedia of the Social and Behavioral Sciences*”, 26 volumes (2001).

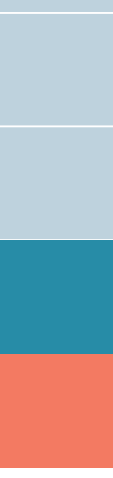
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1

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Wissenschaftszentrum Berlin
für Sozialforschung

D-10785 Berlin
Reichpietschufer 50